Review of the Scientific Program and information relevant to the International Dolphin Conservation Program Act (IDCPA) held at the Southwest Fisheries Science Centre, La Jolla, California, 8 to 11 March, 1999.

By

Donald B. Olson
Professor Meteorology and Physical Oceanography
University of Miami

Introduction

This review encompasses the 25 February 1999 Draft of the NMFS report,the supporting documents on the programs, review letters solicited by NMFS on the components, and letters of review from the Marine Mammal Commission and Inter-American Tropical Tuna Commission (IATTC). It also involves perspectives gained from presentations to the review committee at meetings held in La Jolla on March 8-11, 1999. This review will begin with an overview of the effort from my perspective. A detailed review of the areas relevant to my expertise; physical oceanographic setting and variations, aspects on the analysis of the dolphin habitat in the Eastern Tropical Pacific, the sampling programs for assessing dolphin abundance, and the population modeling used to arrive at a finding. The portions of the program dealing with stress and cryptic mortality will only be commented on in passing in relationship to the modeling. The technical comments on these issues are preceded by a comment on the structure of the document.

Structure of the Document

The sections in the report seem out of order. In the Act calling for the research there are two points, 1) Abundance and 2) Stress on dolphin. The report starts with abundance then switches to the stress studies. It then returns to the abundance question with an analysis of habitat and finally a model estimate of population trends. It seems logical to move the stress section to the end of the model discussion. This allows the abundance issues to be discussed in sequence. The section on the decision framework adds an interesting approach to the IDCP mandate. The contents should be reorganized to put some of the decision section at appropriate places in the text. A smaller section on the details of the decision work should then come between the model and stress sections (see comments on the decision section below).

The Eastern Tropical Ecosystem

The report attacks the problems posed by Congress in a logically and straightforward manner. The details of the region environment are found in two Primary Research Documents prepared by Drs. Fiedler and Gerrodette covering the habitat variability in the ETP and the 1998 dolphin abundance estimates respectively. The reviewer also received a set of reprints from these authors and others discussing the background behind the work. Here these two elements of the report and science background will be reviewed together. This is because they are fundamentally linked. That is the habitat analysis depends on the abundance surveys and a set of physical data sets. The finding concerning the habitat is crucial for the finding of a lack of evidence for habitat variations that might explain long term shifts in dolphin abundance. Finally, the abundance data are at the heart of the modeling behind the finding here and the future one in 2002. The section 5.4 on the older surveys should also be moved up into Section 3.0 as discussed above.

To begin, this work is of high quality and has been done with a careful choice of sampling and analysis tools. In general, the finding relative to the physical environment is valid within the scope of the data, i.e. there is no evidence that there has been any major change in the habitat regime in the ETP that would suggest a decline in dolphin stocks. However, there are some worries about the data in relationship to abundance over time. This concern also impacts the model results because of its use of this data.

The difficulty involves the large variations in the absolute abundance estimates (App. 2, Figs. 1,2). The interannual swings in the abundance estimates can not be biological as pointed out by the authors in their published work and in App. 1. This is not discussed in the main text. Neither do the figures supplied in the text give the reader much of an appreciation of the abundances in the context of the ETP environment. The following suggestions are made:

- Change the title of section 3.0 to just reflect abundance estimates in general, i.e. the 1998 survey, MOPS and the earlier survey Work, and the Tuna Commission's TVOD indices.

- Include an opening discussion of the MOP and 1998 results and the variations in absolute abundance estimates. Describe the possible reasons for the interannual variations in the estimates. This should carefully define the area used to designate the dolphin stocks. Spotted dolphin, for example, are Trans-Pacific so the stock definition is highly relevant here. This section should include a plot of the absolute abundances over time without the model and TVOD data. In relationship to comments concerning habitat below it would be appropriate to plot the abundance data on the same plot as the dolphin habitat availability (Fig. 5, Fielder white paper).

- Explain the possible reasons for the variations in the estimates, i.e. migration or changes in dolphin availability to the survey. Make it clear here that the interannual changes can not be due to local biological factors in the population such as birth and mortality.

- Point out the importance of carrying out further surveys in order to further clarify the distributions of dolphin abundance and identify possible linkages between variation in the estimates and the ETP habitat (see below).

- Expand Fig. 1 or include another figure showing the survey results. Figs. 2 and 3 from Gerrodette's white paper would be appropriate. This shows the most extensive accomplishment in coming up with the March 1999 findings.

The sections on the TVOD data on mortality and the accompanying index of abundance can be moved up to Section 3.0 and put in a paragraph starting out "Other relevant data on ..." This eliminates the structure in the original draft with its "primary" and "secondary" portions. Also, in an effort to at least acknowledge the concerns of the Tuna Commission, it might be wise to mention some of their problems with the TVOD data. These involve changes in the properties of this data set over time that might reflect variations in the way the data were collected. The data also seems to have the index following the effort rather closely. The report should at least mention these concerns since problems in the TVOD data will significantly impact the model used in the report.

The fisheries derived TVOD data are a major contributor to the results in the model in the report. The TVOD data are the only data set that has the scope to provide a long-term trend in ETP dolphin stocks. The inclusion of the TVOD in the report has created a high degree of angst in the Tuna Commission. They point out the index has changed markedly with the change in fishing practices over the last decade. In particular they suggest that there was a shift in the TVOD after the 1992 La Jolla meeting that lead to differences in how observers worked. This is made worst by the increased use of bird radars that allow the larger boats to spot possible schools and decide to steer towards

them by helicopter operations at ranges of up to 20 nm. The timing of this corresponds to a drop in the TVOD indices from 1992 to 1993. Curious also is the fact that the TVOD remains almost constant and has a much reduced variance after 1992. The Commissions search time index for yellowfin tuna, that agrees with their cohort analysis, and the TVOD are also tracking effort. This is typically worrisome in fishery data. In conclusion, the report needs to document the concerns of the Commission concerning the TVOD and its use.

At the end of the abundance section there needs to be a discussion that provides an overall assessment of the problem with providing accurate abundances with which to make a decision. In general, here and elsewhere it is important to make Congress aware of where uncertainties lie and how the effort underway is trying to address these unknowns. This is the place to stress the role of the surveys in providing a better picture of ETP dolphin stocks.

The comments above carry over to the habitat analysis (Sect. 5.3). The reviewer agrees with the validity of the finding at the end of page 10. It would be appropriate to again provide some illustrations depicting the habitat structure in the ETP and its variability. A suggestion for a figure showing abundance and an environmental index is made (Fig 1). The second paragraph of this section should be revised and expanded slightly. The suggestion is to incorporate Fig. 4b from the Fiedler white paper. The patterns in the spinner dolphin habitat can then be contrasted with the signals in abundance in the previously suggested figure. The reviewer would here speculate that the figures show a coherent pattern in habitat and abundance with low abundance at El Nino peaks and high abundance during La Nina. The MOPS 1988 and SPAM 1998 show a compression of the habitat suitability into the coast of Central America during La Nina phase in at least these two incidences. While one resolution of an ENSO cycle and a few scattered other points are too little data to produce a significant correlation, this suggests that a longer data set may come up with a significant spatial/temporal set of patterns. Understanding these patterns might assist in reducing the high variance within Surveys and large shifts between surveys.

The Decision Analysis

The decision framework laid out in the report is interesting and at least to this reviewer novel. If not allowed to become a pedagogical crutch this type of careful attention to the questions one wants to answer with an analysis is worthwhile. Some of the early part of Section 6.0 is very introductory to all of the other portions. To sandwich it in between the abundance and habitat material and the stress issues seem out of place. Placing it between the former materials and the model analysis also awkward. The suggestion would be to consider placing some of it prior to the abundance section as part of the discussion of initial strategy. The discussion in 6.1.4, however, suffers from the difficulty that it makes reference to the model and introduces variables that are not defined until the model section. See for example, the introduction of Rmax on the top of page 14. The final criterion in the middle of page 14 is hard to understand. The choice of 1% probability in criterion 1) is consistent with a 99% confidence that the stock will not go extinct. The second to choices, however, are not as obvious. The discussion at the end of these criterion are impossible to follow without a better history of the tuna/dolphin controversies. The Panama agreement and the PBR criteria need to be put in context somewhere in the document. In conclusion the decision analysis needs to be more carefully introduced such that it provides a rationale for the abundance and habitat sections and does not prematurely introduce model variables that have not been discussed yet. These model application portions might be placed at the end of the model section.

The Population Assessment Model.

The population model and the set of estimations made with it are interesting from both a theoretical and an application point of view. Dr. Wade and the others involved with producing it should be highly commended. The model is a Leslie matrix formulation with added mortality outside of the matrix demography. The report says it is the same as Wade (1994), but it applies the fisheries

mortality differently. The reviewer would have chosen the 1994 implementation at first look. For the nonmodeler, the Leslie model is a discrete, i.e. one step per year, model that marches a population through age classes with some survival rate between classes and a reproduction term that allows mature animals to repopulate the zero year class. As discussed in detail by Caswell (1989) they are an approximation whose success depends on the time scales of generations relative to the class steps and the manner in which survival and in this case mortality are handled relative to reproduction. Given the long lives and slow growth of the two dolphin populations of interest the model is a natural first choice. Likewise the Bayesian methods used for projection of the model onto the data is one of several state of the art choices. The reviewer can not comment on the latter's implementation other that to say that sensitivity runs that the committee asked Dr. Wade to run during the review suggest that it is reasonably consistent. Clearly the population model is as complicated as one can expect to apply to the data. The techniques to apply it to the data are appropriate. There are several questions to pose, 1) are the assumptions made in the formulation of the model robust versus other choices, and 2) are the data inputs sufficient to allow the model to really produce a result that is usable in the decision framework? The third is the choice of variables to apply to the decision criterion in the report.

To address the first question, there are other approaches that should at least be explored. The model assumes it is following a single stock without migration and that there is no spatial structure. Under the Sustainable Fisheries Act the mandate is to manage fisheries from an environmental perspective. The habitat work of Fiedler's and the spatial surveys suggest that there should be an effort to explore these issues. As discussed above in relation to the abundance and derived habitat analysis there are other factors that need to be addressed. While the current model is age structured it does not take into account the well documented sex bias in the fishery deaths that occurred in the past. For species that take order of 10 years or longer to reach sexual maturity and experience a bias in fishery death to females this issue is still important in the 1991-98 analysis since the first cohort of females without the heavy fishing mortality is just now beginning to reach maturity. As pointed out in the report the maturation of these cohorts may provide a recognizable signal in the upcoming surveys. The investigators should try to model the possible manifestation of the expected increase in population growth potential. Finally, it is probably wise that the effort branch out to include some additional modeling approaches. A spatially explicit set of models are suggested to follow up on Fiedler's work and try to understand the large variances that occur in the survey data. In particular, the issue of migration of the spotted dolphin stocks should be considered.

The next issue is the quality of the data sets for inclusion in the model. The scatter in the data on the plots in the report suggest caution should be taken in trying to use these data. The patterns in the data suggest that there are other ways of using them. The review team requested statistics from the Tuna Commission on effort and catch by set type. For example the 1986-88 change in absolute abundance in the survey data corresponds to patterns in the distribution of effort in the tuna fleet. The patterns, therefore, in both the commercial data and the research surveys suggest that there are significant patterns in the ETP habitat that are responsible for the locations of the dolphin populations in different years (Reilly, 1990; Reilly and Fiedler, 1993, Fiedler, white paper). Similar patterns occur in the fleet effort. In 1986 the heavy effort by class 6 boats extended out to 130 W. This western area of effort diminishes in 1987 and disappears completely In 1988. This same period corresponds to the increase in the absolute abundance estimates and to the habitat changes relative to the shift from El Niño to La Nina conditions. It is the reviewers contention that the variances can be reduce substantially if the spatial patterns are taken into consideration; i.e. understanding the spatial patterns and their relationship to the ETP environment should allow a model effort that is not plagued by high variances. The investigators are already well on their way to quantifying the spatial patterns, the next job is to take them into account in the modeling. The final point with the model is the choice of Rmax and mu as the decision variables. The reviewer struggled with both of these. The Rmax is derived from the characteristic equation from the Leslie matrix and is related to the first eigenvalue (Caswell, 1989; Wade 1994). It is a measure of the population entering each fecund age class and their fecundity. In the very crudest approximations

(two age classes, immature and mature) Rmax is the natural log of the survivorship of the immature times the fecundity. The additional mortality, mu, is diagnosed as an additive term to the fisheries mortality. It seems to the reviewer that any errors in the data used to start the model off in 1991 becomes part of mu. The introduction of mu to the model is clever, but it is important to understand its sensitivity. For the decision process the ratio of Rmax to mu is considered. This bothers the reviewer from the perspective of units alone. With a week to look at the model, it is possible that this impression is incorrect. Overall, the model is an excellent piece of work and a good start on the route to a final statement in 2002, but the report should reflect some reservations about its findings at this stage. The model description and conclusions should stand where they are, but enough caution should be conveyed here to allow an easy rebuttal given further modeling and data for the 2002 report.

References:

Caswell, H., 1989. *Matrix Population Models: Construction, Analysis, and Interpretation*. 328 pp., Sinauer Associates, Inc., Massachusetts.

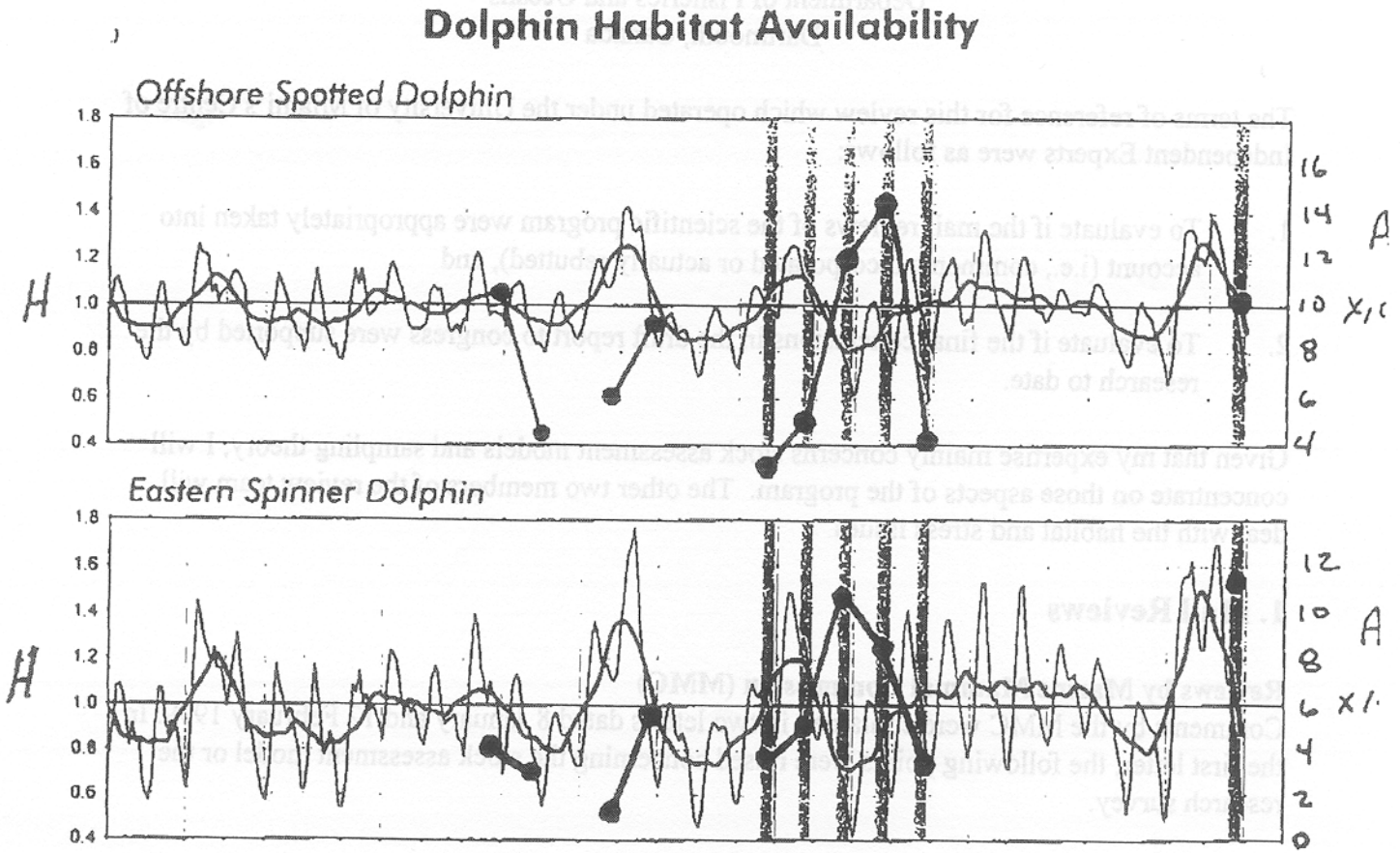

Fig. 1 Dolphin habitat availability (H) as derived by Fiedler (white paper) and The absolute abundance estimates for offshore spotted and eastern spinner dolphin. The thin lines for H denote monthly values while the thick line is a 13 month runing mean.